THE MUTATION FACTOR IN EVOLUTION: WITH PARTICULAR REFERENCE TO OENOTHERA. By R. RUGGLES GATES.[1] Since DeVries discovered the two aberrant forms of *Oenothera Lamarckiana* that he called *brevistylis* and *laevifolia* at Hilversum in 1887, the genus *Oenothera* probably has been studied more intensively than any other similar group of plants. As evidence of this analytic activity a literature of some five hundred titles has been produced. Every botanist appreciates the numerous facts that have been brought to light regarding the classification, distribution, gross morphology, cytology and genetics of the genus, and owing to the volume of the publications everyone is thankful when compilations are made. DeVries has brought together the greater part of these facts in Die Mutationstheorie and Gruppenweise Artbildung. The volume under consideration supplements these two works by a fairly complete consideration of the taxonomy, cultural history and cytology of the group. In addition the author brings together the data from his own extensive pedigree culture, and urges, rather didactically at times, the conclusions he has drawn from them.

The book purports to be a reconnaisance of the theory of Evolution by Mutation, it really is wholly a consideration of the biology of the Oenotheras. The author rests his case on the one group of facts. DeVries did not make this mistake; for, as Cervantes says, "It is the part of a wise man to keep himself today for tomorrow, and not to venture all his eggs in one basket." DeVries did indeed lay great stress upon his work with the evening primroses, but he did not overlook numerous other props for his hypothesis,— props so sturdy that in the opinion of some, the Oenothera investigations might be disregarded without weakening the edifice. Perhaps few biologists adhere strictly to DeVries' views of Evolution. In particular it might be mentioned that he did not go far enough in distinguishing between germinal and somatic variations, and that he has not kept pace with the facts regarding inheritance. At the same time, it must be admitted that in addition to the great stimulus to experimental biology that his work effected, DeVries made two great general contributions. He showed the frequency with which germinal changes of comparatively great size occur, and why they are not swamped by intercrossing. But these generalizations make no new Evolution theory. They merely extend and modify Darwin's ideas insofar as these new facts tend to change the emphasis the latter placed upon particular types of variation.

These changes in viewpoint may be made with total disregard for the Oenothera work. In fact, perhaps few angiosperm genera could have been selected which are so fundamentally unsuited for genetic work from which broad conclusions are to be drawn as Oenothera. As Gates shows, numerous aberrant types of chromosome distribution occur at gametogenesis. Presumably many of the daughter cells

[1] London. Macmillan, 1915. pp. I–XIV + 353. Figs. 114.

formed by such irregular division are aborted. Thus there is a selective elimination of potential gametes. Further, many of the pollen grains, and possibly the egg cells, are not functional. Again, an extremely large percentage — often the majority — of the zygotes formed are not viable. This will be apparent to anyone who takes the trouble to make germination tests of Oenothera seeds. Even after discarding the many seeds that casual examination shows to be worthless, it is seldom that over sixty percent of the remainder produce mature plants, and the germination may drop as low as five percent. Now, it is quite likely that the only useful laws of heredity will be those which like the laws of physics and chemistry are mathematical descriptions of cycles of events from which predictions of what must occur under like circumstances may be made. Is it strange then, that many biologists are cautious when asked to accept as a basis for such descriptions, breeding results from plants like the Oenotheras where only a small portion of the facts can be known owing to the immense number of potential plants lost through the abortion of both zygotes and gametes? It is like asking a chemist to accept theories as to the structural formulae of organic compounds upon which only determinations of nitrogen and oxygen have been made.

These facts are boldly disregarded by the author in his chapters on "Hybridisation and Hereditary Behavior" and "The Relation between Hybridisation and Mutation." The Oenotheras, he says, have four main types of hereditary behavior: "(1) mutation crosses, (2) Mendelian splitting, (3) blending and modification of characters, and (4) twin hybrids." Much of the discussion under these heads is a severe arraignment of Mendelism, but the author's contempt for the Mendelian theory of heredity is not that bred of familiarity. If the reviewer has not misconceived matters, the author's idea of Mendelian segregation is enticingly simple. If when two organisms are crossed, The F_1 generation is uniform and the F_2 generation comprises two types in the ratio of three to one, the inheritance is Mendelian. No circumstance whatever may modify the definiteness of these phenomena under pain of their disqualification as examples of Mendelian inheritance.

It would be rash to assert that Mendelism even in the broadened sense with which the word is used today, covers all types of inheritance. Correns' and Baur's experiments on chromatophores and Goldschmidt's work on the gypsy moth indicate the possibility of inheritance through the cytoplasm, while Mendelian data parallel chromosome distribution.

On the other hand, it is even more bold to assert that inheritance in Oenothera is definitely nonmendelian. They cannot be placed in either category with certainty, but it ought to be emphasized that no single fact discovered by those who have made pedigree cultures of the group, precludes a Mendelian interpretation. Gates' arguments against Mendelian interpretation of heredity in the evening primroses reduce to four: (1) It has been definitely proven that *O. Lamarckiana* is not a hybrid; (2) Constant ratios of the usual Mendelian type are not found; (3) Splitting often occurs in the F_1 instead of the F_2 generations, sometimes with the production of two types unlike the parents (twin hybrids); and, (4) Chromosome differences appear.

In regard to the first point, it may be said that the finding of a single herbarium specimen hardly constitutes proof that the plant in question is or is not a hybrid The second point is covered by our remarks on the selective elimination of zygotes and gametes. The third argument overlooks a fundamental tenet of Mendelism. Crosses split first in the second hybrid generation, solely when the eggs on the one hand and the sperms on the other hand entering into the so-called F_1 generation were of the same factorial constitution. The mere act of producing a cross does not necessarily make the next generation an F_1 generation. The fact that *O. Lamarckiana* gives off variants is to many an indication that a cross between it and another form does not produce an F_1 hybrid. The frequency or infrequency with which the aberrant types appear proves nothing as long as we are ignorant of the potentalities of the non-functional gametes and zygotes. With this ignorance to contend with, the variability of the ratios is even an argument in favor of segregation rather than mutation, for it must be remembered that the same types continually reappear, whereas by DeVriesian theory mutations are equally likely to occur in any direction. The fourth argument of the author is really a question of terminology. If the behavior of the chromosomes is the efficient cause of Mendelian phenomena, then even aberrant mitoses at reduction are in the broad sense Mendelian. They hold possibilities of variation without that true germinal change which may be pictured as a chemical reconstitution independent of the mechanics of cell division.

In calling attention to these points, however, the reviewer does not wish to have it understood that he denies mutation. Wide variations and narrow variations as opposed to mere adaptive fluctuations certainly appear, and some of these variants may have been produced independently of "slips" in cell division. But it seems to him unwise to make the case rest upon the Oenothera data. The author speaks truly when he says that "biology has passed the stage when single evolutionary factors, no matter how insistently urged or how brilliantly advocated, can be held accountable for the great diversity of life which we see around us." — E. M. East.